## LETTERS TO THE EDITOR.

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### The Habitability of Mars.

INASMUCH as Dr. Wallace has sent me his book through his publishers, as I gather from the wrapper—though it is not so expressed—I suppose it is incumbent on me to acknowledge it, since he clearly expects some sort of reply. The effect of its perusal is to show me again how cogent is the argument for the habitability of Mars, for only by many misstatements of fact, wholly unintentional, of course, can Dr. Wallace make out even a seeming case upon the other side. A physicist will not need to have these errors pointed out to him, but as most readers are unable to correct them for themselves it may be wise to instance a few to show how his house of cards tumbles down in consequence.

On p. 22 he quotes from Miss Clerke to prove that the cap could only supply 2 inches of water over the irrigated districts. Let us assume her own estimate of snow deposited, and merely correct her mathematical and topographic mistakes. She states the maximum area which the cap covers to be 2,400,000 square miles. Now the south cap comes down to 36°·5 latitude on the average, and an easy calculation shows this to occupy 11,330,000 square miles, or to be more than four times as great. Next, she supposes the natural dark areas of the planet to be irrigated, which they are not, mistaking them for the canal system, which, instead of 17,000,000 square miles, covers, oases and all, only about 4,750,000 according to our measures, remembering that the whole of it is not watered from one cap. By combining these two corrections we find, not 2 inches of water for each bit of ground, but 2½ feet, and this according to her own estimate, which there is no reason to suppose not to be two or three times too small. So that it is the argument of Dr. Wallace, and not the cap, that fails to hold water.

An equally fatal flaw affects Dr. Wallace's argument for temperature. Here he bases his deduction on a misstatement of Prof. Poynting. Prof. Poynting states that in my paper on the mean temperature of Mars I took no due account of the blanketing effect of air. Not only did I expressly take it into account, but I did so in the only way it can correctly be taken, not by hypothesis, but by direct appeal to what takes place on earth under a clear and under a cloudy sky by night; and I am glad to know that in a paper he has sent to the *Phil. Mag.* on the subject Prof. Very, the bolometric authority on matters of temperature to-day, agrees with both my method and my conclusion for Mars, and points out where Prof. Poynting's calculations are fallacious.

Another omission is no less telling. Dr. Wallace apparently is unaware that Prof. Very's bolometric determination of the moon's heat, which for delicacy surpasses any previous ones, makes the temperature on the moon during the lunar day reach 356° F. above Fahrenheit zero.

Many more such misunderstandings might be mentioned occurring throughout the book, such as where, from not giving its context, he makes me appear to say that water-vapour is one of the heavier gases, which, of course, I did not.

Again, his theory, taken from Chamberlin, that the interior of Mars can have completely lost its heat in the very process of contraction, and yet later have suffered a meteoric bombardment sufficient to give it a heated outer layer, is mechanically whimsical, not to say impossible. For it can be shown that Mars could not have captured any meteoric swarms not substantially travelling in its own orbit when it coalesced into a planetary mass, and any meteors subsequently encountered could only have fallen on it as it passed through a swarm, yielding a relatively insignificant amount of matter. Any such effect would be even more pronounced on the earth, of the occurrence of which there is no evidence.

Misstatements cannot be too carefully avoided in science, especially when a man, however eminent in one branch, is wandering into another not his own. Dr. Wallace, whose intentions are of the highest, will appreciate this. Indeed, if criticism were confined, as common-sense counsels, to those versed in the phenomena, we should hear very little about the inhabitability of Mars.

Boston, March 6. PERCIVAL LOWELL.

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DR. J. W. EVANS'S letter in NATURE of February 27 seems to invite notice from me in respect to three of the subjects with which it deals.

(1) As regards temperature. In most physical problems temperature may be regarded as a single definite measurement, which I understand to be Dr. Evans's point of view; but this ceases to be legitimate in molecular physics whenever the behaviour of an individual molecule comes under consideration. Temperature has then to be recognised as not one, but many, measurements, chiefly of two groups of activities, one group associated with the events that go on *within* the molecule and are in touch with the activities of the æther, and the other group mainly concerned with the journeys of the molecule through space and with one section of the events that occur during each of the encounters to which it may have to submit. Dr. Evans will find this subject referred to, and partly dealt with, at p. 76 of the *Astrophysical Journal* for July, 1904, or in the *Phil. Mag.* of the preceding month. On the other hand, in molar physics (as also in the kinetic theory of gas as usually treated) we have no occasion to deal with individuals; we are only concerned with swarms of molecules acting on one another and changing their behaviour so frequently that the activities of or within the molecules come into operation in too rapid succession to be distinguishable. All that we can then detect is that these numberless activities furnish an average outcome of energy which fortunately is (except in certain critical instances) sufficiently steady to admit of measurement, and is then what we call the temperature. But this jumbling together of unlike activities is not admissible when the question is about individual molecules—as when our object is to learn the conditions under which the lightest gaseous molecules of an atmosphere, which are those most violently tossed about, can occasionally and *one by one* drift away from their atmosphere.

(2) The question whether we can know that Mars is unable to prevent the escape of water is in effect almost the same question as whether we may trust the evidence that helium is in process of escaping from the earth, inasmuch as the dynamical conditions in these two problems are nearly identical. The evidence in the case of helium, so far as it was known eight years ago, Dr. Evans will find on pp. 369, &c., of the *Astrophysical Journal* for June, 1900. It should be added that the discoveries since that date about helium have materially strengthened the evidence then available.

(3) Dr. Evans bases an argument on the early state of the earth, which he thinks could not have been followed by the presence of water in modern times if some molecules can now escape from a planet in the way I have supposed. This, I believe, is a mistake. In the remote past the potential of attraction of the dilated earth of those days may have been, as supposed by Dr. Evans, so much less than now that multitudes of molecules now on the earth were not then upon it. So much may be conceded. But then, as now, these molecules were under the influence of the sun's attraction, and did not range beyond a ring round the sun, in which the earth also travelled—like the rings of Saturn or the asteroids of the solar system. Afterwards, when the earth shrank and the potential of its attraction rose to near its present amount, such of these molecules as encountered the earth were unable to escape again and we now find them upon the earth. There is therefore no such conflict as Dr. Evans supposed between this possible past and the argument I have based upon observed facts, viz. upon the absence of all the gases of its atmosphere from the moon, and on the escape from the earth of molecules of hydrogen and helium which is still going on.

The more deductive method of investigating the escape of gases from atmospheres, without the premisses from

observation of which I have latterly made use—which deductive method I attempted in former times, and upon which others have relied since—will, I am persuaded, continue to be incompetent to deal with this real problem of nature unless man's knowledge of molecular physics receive such unhoped-for accessions as will enable him to trace the history of single molecules. Meanwhile, what I advocate is that we avail ourselves of the mixed method, which introduces data established by observation to supplement the deductive method at the point where the deductive method fails. G. JOHNSTONE STONEY.

30 Chepstow Crescent, W., March 6.

*Postscript, added March* 13.—NATURE of yesterday's date announces the last supposed spectroscopic detection of water vapour upon Mars by one of Prof. Lowell's assistants. Observations of a like kind had been recorded by Sir Wm. Huggins and Prof. Vogel, and the wave-lengths of three of the lines observed were measured by Vogel, two of which may possibly be water lines recorded by Rowland, but not the third.

On the other hand, Campbell and Keeler in a better climate did not see them. Now, however, they seem to have appeared again. This would be the behaviour of a very variable coloured vapour like $NO_2$; and what I should desire is that an adequate study be made of the absorption spectra of the several such vapours which are unable to maintain themselves in our atmosphere on account of the presence of water, but are presumably to be found on Mars if water does not exist on Mars, and which if present will account for the orange colour of large tracts upon that planet, and for the variations of its colour at different seasons which are conspicuous.

It is to be regretted that the observers to whom we owe so much—from Schiaparelli to Lowell—have kept in view only one of the competing views as to the state of things on Mars instead of at each step considering them both, especially as the one they have preferred is that which some physicists have felt to be the least probable.

G. JOHNSTONE STONEY.

## The Isothermal Layer of the Atmosphere.

LIKE Dr. Chree (p. 437) I have had experience of the vagaries of self-recording instruments, but I have generally been able to trace them to some remediable defect in the instrument or to the ignorance or carelessness of those who use them. I fancy that the man who constantly uses a certain instrument, and uses it intelligently and not by mere rule-of-thumb, has a fairly correct notion of the magnitude of the errors to which it is liable. If not, what reliance are we to place on any instrumental observations?

It is quite natural, however, to doubt the observations, and when this investigation first commenced I confess that I did the same. Now that hundreds of ascents have been made with different instruments, in different countries and in widely different circumstances, and all the results obtained are in striking agreement, such a view seems to me to be quite untenable. It is true that different instruments sent up with the same balloon have given widely different temperatures, but the results have been published, not concealed, and the instruments improved. I ascribe these discrepancies, which are the exception, not the rule, to solar insolation, which we avoid in England by making our observations after sunset.

With regard to the general question, the difficulties of registering a true temperature are two:—(1) stagnant and unmixed air which may be at different temperatures in different parts of the same garden; (2) the proximity of bodies of large thermal capacity, which by radiation and convection mask the true air temperature. Kites and balloons when they have left the earth are free from these errors, excepting that No. 1 applies to a balloon which does not burst when swimming at its highest point. Since, however, stagnant air does not matter provided sufficient time is allowed, and in this case time is allowed, I do not see what source of error there can be save solar insolation.

My belief in the accuracy of the thermometric results obtained in England is based on inference from the following facts. If a good trace, together with the constants of the instrument, is given to two persons, they, working quite independently of each other, will get practically identical results. If the trace and instrument only be given to two persons, they, each calibrating the instrument for himself, will obtain similar results for temperature within the limits stated, but the agreement for height may differ by a kilometre or more in the higher parts. Hence I believe in the accuracy of the temperatures, but do not claim any great accuracy for the heights.

Now with reference to Dr. Chree's questions.

(1) Each station is held responsible for the accuracy of its own results, and I am not acquainted with the routine pursued at each individual station, but the general practice certainly is to test each instrument in spirit cooled by solid $CO_2$ both before and after each ascent.

(2) Answered above.

(3) No. The instruments used on the Continent are expensive, and being heavier require a more expensive balloon, and we have no funds with which to meet the expense, especially when it is remembered that balloons and instruments in England are lost about three times out of ten. We hope that this will be done on the Continent before long. W. H. DINES.

## Classification of Secondary X-Radiators.

IN NATURE of February 13 there is a letter by Dr. C. G. Barkla and Mr. C. A. Sadler in which the authors divide the elements—according to the qualities of the secondary X-rays emitted by them—into four groups founded upon the atomic weights, without consideration of any other quality of the element. It may be of interest to mention that practically the same classification was given by me as early as 1896 in the *Naturwissenschaftliche Rundschau* (vol. xi., p. 485), and that this classification was also dealt with in a treatise published by Prof. Voller and myself in the *Annalen der Physik und Chemie* (vol. lxi., p. 88, 1897). To this treatise there is added a table printed directly by the secondary rays of a great number of elements, and this shows not only the great difference between the elements of the different groups, but also the agreement in the behaviour of the various elements of the same group. B. WALTER.

Hamburg, Physikalisches Staatslaboratorium, March 2.

## Gods and Godlings.

LEST some readers should infer from your obituary note on Sir Denzil Ibbetson (March 12, p. 443) that this distinguished anthropologist invented the word "godlings" for the rural deities of India, it is worth noting that "godling" was good English in the sixteenth century, and has never been allowed to drop. The Philological Society's "New English Dictionary" quotes Lambarde's "Perambulation of Kent" (1570–6) on raising altars "to this our newe found Godlyng"; and examples from Drummond of Hawthornden, Dryden, and Peter Pindar show the convenience of the word. Coleridge preferred "godkin" for a minor deity with masculine attributes, but sanctioned "goddessling." Charles Colton in 1675 permitted a certain cult of "little Goddikins"; Coventry Patmore regarded "godlet" as the more dignified appellative. Anthropologists have therefore had a fairly ample choice; but it should be added that in some of the above examples, at least, Dr. Murray and his coadjutors suspected a "jocular" intention.

DAVID PATRICK.

Edinburgh, March 14.

## Tabulated Values of Certain Integrals.

IN NATURE, October 24, 1907 (p. 639), the integrals $x=\frac{k}{2}\int \cos u^2 du$ and $y=\frac{k}{2}\int \sin u^2 du$ are given. I shall be grateful if any of your readers can inform me where I can obtain tables of the numerical values of these integrals, or any other tables that will reduce the labour of the numerical calculation of them. C. E. ADAMS.

9 Telford Terrace, Oriental Bay, Wellington, New Zealand, January 18.